Fall in the Sea, Eventually? A Green Paradox in Climate Adaptation for Coastal Housing Markets

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Abstract

Integration of science into policy is a primary challenge for climate change adaptation. Yet, when communi-
cation of climate science results in a policy signal without concurrent political action, the economic incentives
created by the expectation of policy change may have unintended consequences. We examine the effect on
new housing development resulting from a scientific report by a regulatory agency mandating coastal com-


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1. Introduction

Preparation for the impacts of climate change has received increasing attention in recent years from scientists and policy-makers globally [1, 2]. Calls for development of adaptation science [3, 4], research into overcoming impediments to adaptation [5] and better communications of potential risks [6] are progressing, however on-the-ground adaptation planning has been incremental at best [7, 8, 9] when substantive change is needed [10]. The current lack of comprehensive adaptation policies stems, in part, from the uncertainty of climate impacts [11], the timescales over which future impacts may occur [12], and budgetary and political constraints [7]. Moreover, adaptation is fundamentally a regional or local problem as climate impacts are expected to vary spatially [13, 14, 15, 16], necessitating a merging of location-specific science and proactive decision-makers to effectively tackle these critical issues.

Public policies in general are designed to elicit a change in behavior to mitigate or lessen the effects of a perceived problem. In the case of climate adaptation, problems coastal communities face include chronic risk from sea-level rise (SLR) and acute risks from flooding during events like Hurricanes Dorian, Florence and Michael. SLR in the United States (U.S.) is expected to damage and displace coastal development [17], increase nuisance flooding [18] and exposure to acute risks [19], potentially displace tens of millions of people [20] and affect GDP on the order of hundreds of millions to billions of dollars [21, 22, 23]. Due to a lack of national adaptation planning in the U.S., local and state governments are left to decide how and when to adapt to the effects of SLR and climate change. New adaptation-focused policies may seek to limit future development and relocate productive resources away from low-lying areas [24], strengthen building codes [25], or increase investments in natural habitats to provide protection [20, 27, 28, 29].

We investigate how housing development decisions are impacted by a policy signal about future land-use regulations based on the best-available science on the impacts of SLR. Our focus is on a March 2010 NC Coastal Resources Commission report suggesting 1 m of SLR by 2100 should be accounted for in coastal land-use decisions in North Carolina (NC), a U.S. state with a large coastal plain below 1 m in elevation (> 5,000 km²) [30, 31]. The NC State Legislature made national headlines 29 months later (August 2012) when lobbying efforts by coastal business leaders and stakeholders resulted in a moratorium on creating new policies based on the 1 m SLR forecast (H.B. 819) [32]. Our hypothesis is that since developers did not know *ex-ante* the nature of future regulations or which parcels of land would be more heavily regulated, the policy signals may influence the timing of housing starts and the overall supply of at-risk coastal housing. Using a quasi-experimental estimation strategy, we find evidence that these signals, without concurrent policy implementation, generated incentives that accelerated the timing of development decisions, potentially increasing the value of housing stock vulnerable to damage from SLR before new regulations could come into effect.

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1 H.B. 819 was well documented in the media when it passed the NC State Legislature in August 2012 (ABC News, Scientific American, The Guardian) and returned to public attention in 2018 due to Hurricane Florence (CBS News, New York Times, Washington Post).
Previous research has identified the potential for unintended consequences from climate-related environmental policies. The primary example is the green paradox, which occurs when anticipation of a future policy intended to mitigate carbon emissions induces suppliers to extract fossil fuels more quickly, thereby increasing emissions in the short term. The potential magnitude of the effect is often debated as empirical evidence ranges from showing little to no effect to impacts that are dependent on fuel type and policy implementation lags. Another strand of the literature focuses on the impact of development timing decisions in response to policy changes. One strategy available to policy-makers is to enact zoning or building regulations designed to decrease property losses in the event of storms, flooding or other natural hazards. For example, Florida implemented stringent building codes in response to Hurricane Andrew to limit the loss of life and property during future storms and evidence suggests the price of vacant land in vulnerable areas decreased significantly after these building code changes. Such changes in land-use regulations are also likely to impact development timing decisions as markets internalize expected policy changes. Titman (1985) demonstrated that greater uncertainty in future prices and demand conditions is directly related to the development timing decision, with more uncertainty increasing option value and thus increasing developer incentives to delay land conversion decisions. Conversely, the threat of a regulatory taking may reduce option value and increase development of vacant land in the short term. In an empirical example, an urban-growth boundary (UGB) policy to reduce suburban sprawl was shown to be effective around Seattle, Washington, reducing the likelihood of development outside the UGB by 28 to 39 percent. Yet, little attention has been paid to how the expectation of new but uncertain policies may affect development decisions in an empirical setting.

SLR has also received recent attention in literature on housing markets. A recent national hedonic assessment suggests vulnerable homes are selling for 6.6 percent less than comparable homes not susceptible to SLR. Simulations of coastal housing market dynamics including beliefs on risk suggest models ignoring heterogeneity in such beliefs may significantly underestimate future price declines due to SLR. Another study in the Chesapeake Bay Watershed suggests SLR on its own is not capitalizing into coastal housing prices but homes vulnerable to SLR without protective structures, like bulkheads or rip-rap, are selling at a 19 percent discount relative to protected counterparts.

Our first contribution is to identify a green paradox arising in a coastal development setting and estimate the potential magnitude of the economic impact. This is the first study, to our knowledge, to do so in the context of adaptation to climate change. The stated goal of the NC SLR report was to mitigate risk in developing land vulnerable to SLR and our results suggest the opposite occurred. Our research design exploits the exogenous timing of two policy signals and spatial variation in jurisdictions that may be impacted by future land-use regulations. This strategy allows us to estimate a plausibly causal relationship between the release and subsequent repeal of the NC SLR report and new housing starts. Since our variable of interest is count data, we use a negative binomial estimation strategy to identify the intention-to-treat (ITT) effect of
the policy signals. Our preferred model utilizes a triple-differences approach where we estimate this effect in coastal NC by differencing away the effects across a neighboring state (South Carolina) and between coastal and inland areas. We find that building permits increased 32 percent after the release of the report and remained elevated after the report’s repeal, albeit statistically insignificant. This result is robust to placebo tests, spatial spillovers and alternative research designs and control group selection. Back-of-the-envelope calculations suggest this effect resulted in $715 million of additional unregulated housing constructed in coastal NC in the 29 months between release and repeal of the SLR report. We also estimate a parcel-level difference-in-differences model in Dare County, NC where 15% of parcels could be inundated by 2080 with losses exceeding $1.2 billion [31]. In these models, we compare starts within the SLR risk zone targeted in the report to those outside the risk zone within the same county. We find compelling evidence that permitting increased 42 percent in areas vulnerable to 1.2 meters of SLR, consistent with our county-level triple-difference results.

The results above suggest that timing and content (or lack thereof) of policy announcements can influence expectations and change behavior. In the context of coastal adaptation policies, especially those likely to impact private durable assets such as land and housing, our results point to the need for a coordinated, joint effort between scientists and decision-makers to avoid paradoxical behavioral responses prior to policy implementation. Our findings suggest significant temporal leakage of housing development decisions, whereby individuals pull forward anticipated regulated future construction to the unregulated present day, thereby increasing the amount and value of property at risk from SLR. This is analogous to a harvesting effect in the context of environmental impacts on mortality (e.g., [49]) but differs in a key dimension. Lack of policy specifics and the indefinite implementation lag suggest that even if the amount of long-run development was not impacted by the policy signal (i.e., homes built in response were “harvested” and simply built earlier than planned), the share of that development in risky low-lying areas or built without new building codes designed to mitigate SLR risk has increased significantly. In other words, policymakers have foregone the opportunity to regulate new construction that occurred as a result of the policy signal and this temporal leakage of development decisions likely increases the SLR vulnerability of North Carolina’s coastal housing stock.

We also add evidence to the literature on development timing decisions and land-use policies. Prior work has supported the option value hypothesis of [41], demonstrating that decreases in current housing supply are likely when there is uncertainty about current regulations [42] or increased variance of development controls [43]. Less is known about how announcements of future regulations will impact these decisions. A primary example is examining the effects of an UGB on development [44]. This policy was announced well in advance of imposition, allowing time for developers to build structures that would be restricted in the future. The author finds evidence of strategic behavior during the implementation lag that may have increased high-density development [44]. Yet even in this case, the land-use restrictions were known with certainty, giving market participants the ability to internalize option value effects. Future regulations
targeted at reducing risk from natural hazards, like those examined here, may solicit a different response. In NC, land-use regulations to mitigate SLR risk were expected, but lacking specific details on development controls or an implementation timeline, and we find that current development speeds up. This suggests that not only does a decrease in option value increases development, but ambiguity in land-use policy signals regarding natural hazard risk can increase undesirable development. This creates an environment where developers may adopt a hedging behavior due to the increased incentive to develop the parcel before more costly regulations are implemented (e.g., [45]). In a policy arena where risks are increasing, our results point to the need for concurrent action from scientists and policy-makers to mitigate potential for perverse incentives and unintended consequences from announcements and implementation lags of SLR adaptation policies.

Lastly, we provide a supply-side complement to recent work on coastal housing market responses to SLR. This is a new and growing area of research that suggests sophisticated home buyers concerns about SLR [46] or a lack of shoreline stabilization structures [48] are producing a SLR price discount for exposed properties. On the other hand, markets with more optimistic buyer beliefs may be overpricing risky properties by up to 20 percent [47]. Our results suggest developers are also updating their behavior in response to SLR and policy information by increasing supply now to avoid future regulations or loss of option value for coastal properties. This suggests that supply changes may also, in part, be driving price differentials in coastal markets with SLR-vulnerable properties. Although beyond the scope of our current analysis, this finding highlights the need for more research on coastal housing market dynamics related to SLR to better inform future land-use decisions and SLR regulations.

This article proceeds as follows. The next section provides context about the policy setting and a simple model of housing development decisions that connects to our empirical model. In section 3, we discuss our data and our empirical framework, including discussion of identification with non-linear panel models. In section 4 we discuss our triple-difference model results, parcel-level model results, and a series of robustness checks. The final section summarizes our findings and the potential policy implications of this research.

2. Policy Setting and Model Intuition

The NC Coastal Resources Commission (CRC) was created by the NC General Assembly as part of the Coastal Area Management Act of 1974 and operates under the Division of Coastal Management (DCM) and the NC Department of Environmental Quality (North Carolina General Assembly, 1974). In 2005, the NC General Assembly passed a bill establishing the Legislative Commission on Global Climate Change and tasked the panel with investigating the possible effects of climate change in NC. The CRC Science Panel developed the North Carolina Sea-Level Rise Assessment Report, utilizing the IPCC’s Fourth Assessment forecasts to evaluate changes in temperature and SLR [50, 51]. The CRC assessment estimated expected relative SLR by the year 2100 for the NC coast using an extended linear trend forecast of 15 inches as a lower bound and 55 inches as an upper bound (Fig. 1). The CRC Science Panel determined that 39 inches
(approx. 1 m) by the year 2100 is the likeliest SLR scenario and began to utilize this estimate in planning discussions for future land-use regulation [50]. Prior to the release of the report in March 2010, there was no public comment period, and no articles pertaining to report findings were published in the major NC newspapers (e.g., Raleigh News and Observer, Charlotte Observer). The unanticipated publication of the CRC Science Panel findings suggests an exogenous information shock - one likely to resonate with coastal housing developers as the DCM has broad regulatory authority in coastal NC land-use planning. Once released, the report was widely publicized, garnering significant media attention and was quickly met with skepticism and methodological criticism. Among the most vocal critics was NC-20, a coalition of stakeholders from the 20 NC coastal counties that formed in 2010 to oppose expanding storm water rules [52]. NC-20 tried to nullify the report by lobbying the state legislature, the DCM and the Division of Emergency Management, all of whom were considering the forecast for land use planning and flood map creation. NC-20’s stated goal was to have the 39-inch forecast reduced and to prevent the estimates from being used for planning and zoning purposes. Willo Kelly, president of the NC-20 group, indicated in an interview with the authors in 2017 that developers were particularly concerned with CRC language that local governments ”shall use” 39 inches of SLR for planning and zoning purposes.

The NC-20, among other groups, successfully lobbied the state legislature and H.B. 819 was passed in August 2012 requiring the CRC to create a new report, forecasting an extended linear trend with historical data and placing a moratorium on utilizing the original report for planning purposes. During this time the NC State Legislature was controlled by veto-proof Republican majorities in both houses and the legislation became law as a result of gubernatorial inaction. While the 2010 report was in the public domain and therefore may have influenced SLR risk expectations, the 2012 legislative action effectively removed the threat of new regulations on coastal land-use and development. The unique circumstances of the report release and repeal, under which expected policy changes were never implemented, allow us to empirically test how coastal developers internalize expected policy changes in housing development decisions. A timeline of these events is provided in Figure 2.

New information about sea-level rise and the possibility of more stringent building regulations have the potential to affect both housing supply and demand. Supply-side effects include increased costs due to more stringent building codes or decreased option value of vacant land due to expected building restrictions. Construction could be inhibited if restrictions reduce land value such that benefits from development are less than development costs. For example, in the most extreme case, zoning changes may prevent construction on a given parcel thereby eliminating option value. On the demand side prices can be affected through a decrease in the expected life of the structure, an increase in expected maintenance costs or by way of increased scarcity rent if areas of land are deemed too risky for residential construction.

Consider the price of a unit of housing in period $t$, which is determined by the discounted income flows provided by that unit:

$$ p = \int_{t=0}^{T} (v - m)e^{-rt}dt, \quad (1) $$
where \( v \) denotes baseline rents, \( m \) are maintenance costs, \( r \) is the discount rate and \( T \) is the planning horizon or expected life of the structure. Suppose that SLR risk decreases the planning horizon \( T \), which would decrease the price of a given property. This occurs if a landowner believes that a property will become uninhabitable in the future due to inundation or some other natural phenomena related to SLR. Next, consider an increase in maintenance costs \( m \) that may result from higher insurance premiums due increased frequency of storms or nuisance flooding. Lastly, consider baseline rents \( v \), which can be affected by zoning regulations that restrict or limit construction and thereby increase scarcity rent. For example, if there are ten oceanfront lots available and five are zoned to disallow residential construction, the remaining five will become more valuable due to rising scarcity rents. Considering these multiple potential effects, the price effect of information regarding SLR-related policy is ambiguous in the current period.

Turning to housing supply, there are two aspects to consider when evaluating a developer’s temporal decision. First there is the option value framework \([41]\). Under the expectation that new regulations will be enacted that restrict future land-use, the option value of holding vacant land decreases. A decrease in option value would incentivize developers to potentially increase housing starts in the current period.\(^2\)

Second, expected future regulatory compliance costs are likely to influence the development timing decision. In our setting, the release of the SLR report represents a signal to developers that new regulations could be imminent but the type, scope, and implementation timeline are uncertain. This created an expectation of future increases in construction costs and more restrictive land use plans.\(^3\) Furthermore, regulatory compliance costs are non-trivial in the residential construction industry, with estimates ranging from 14.5 percent and 54 percent of costs for the construction and land development phases respectively \([54]\). This is consistent with results showing a significant decrease in vacant land values with the implementation of more stringent building standards along Florida’s barrier islands \([40]\). If construction costs are expected to rise in the future, developers will internalize the potential erosion of profits into their dynamic development decision.

Consider a developer’s cost function:

\[
C_t(q) = G(q, X_t, \theta_t),
\]

where \( q \) represents the quantity of housing units, \( X_t \) is a vector of input prices and \( \theta_t \) quantifies the per unit cost of regulatory compliance, all indexed to time \( t \). Consistent with evidence from \([40]\) and \([55]\), we assume \( \frac{\partial C(q)}{\partial \theta_t} > 0 \), whereby development costs increase with the intensity of regulation. If regulatory intensity increases in period \( t + 1 \) then \( C_t(q) < C_{t+1}(q) \)

Consider a situation where an individual developer holds a parcel of vacant land and must decide whether to develop the land in the current period or wait until some future date. Evaluating the intertemporal trade-
off requires examination of the expected profits from developing a parcel of land under current market conditions as compared to expected future profits under uncertain market conditions.\footnote{We assume vacant land to be either property with no structure or one with a structure being considered for demolition and new construction. This assumption is consistent with previous research and is intuitive given that a developer faces a similar trade-off in either scenario.}

Given a developer’s profit function,

$$\pi_t(q) = p_t \cdot q - C_t(q),$$

we assume two possible regulatory regimes, $\theta_t^h$ and $\theta_t^l$, where $h$ and $l$ denote high and low regulatory intensity respectively. Expected profits are given by:

$$E[\pi_{t+1}] = p_{t+1} \cdot q - [\gamma_{t+1}^h \cdot C_{t+1}^h(q) + (1 - \gamma_{t+1}^h) \cdot C_{t+1}^l(q)],$$

where $\gamma_{t+1}^h$ represents the probability of a high regulatory regime in $t+1$ ($\theta_{t+1}^h$), whereby $C_{t+1}^h(q) > C_{t+1}^l(q)$.

This framework is similar to Riddiough (1997), where an increase in the probability of a regulatory taking decreases the hurdle value at which a parcel should be developed, increasing the incentive to develop the parcel before more costly regulations are implemented.\footnote{Riddiough (1997) also shows that the value will decrease below that which is optimal to hold the land undeveloped.}

Following this previous theoretical research, we assume the developer converts a parcel of land at period $t^*$ such that,

$$e^{-rt^*} \pi_{t^*}(q) > e^{-rt} \pi_t(q)$$

for all other $t$.

In our empirical setting, the intertemporal decision of when to develop a parcel of coastal land may be impacted by the release of the CRC SLR report. As noted above, incorporating SLR into land-use planning would likely increase costs due to more restrictive building regulations or reduce option value through the potential for a regulatory taking. This could lead to two potential outcomes in terms of optimal development timing: 1) no change in $t^*$; or 2) development decisions are pulled forward into the current period. The former could be true if developers anticipate future SLR risks prior to the report and make land-use decisions under the assumption of more restrictive future regulations (i.e., high $\gamma_{t+1}^h$). The latter could be true if developers internalize the CRC report as a regulatory signal and update expectations regarding the probability of increased regulatory intensity (i.e., $\frac{\partial E[\pi_{t+1}]}{\partial \gamma_{t+1}^h} < 0$). This would likely result in temporal leakage of housing development, moving $t^*$ forward in time as developers evaluate the trade-off between developing a parcel today under known regulatory conditions or in a future period with the potential for more regulatory intensity. Our empirical research strategy seeks to test the impact of a policy signal on developer expectations of future regulations as observed through changes in timing of housing starts (i.e., $t^*$).
3. Data and Methods

We obtain county-level building permit data on housing development for NC, South Carolina (SC) and Virginia (VA) from the Department of Housing and Urban Development, compiled by the U.S. Census Bureau through the Building Permit Survey. The data span January 2007 to December 2016 and include monthly counts of single-family residential housing starts and an estimate of average construction cost determined by the local permitting office [56]. Counties are included in our analysis if all permit offices in the county are surveyed in a given month during the sample period. A significant number of counties report issuing 1-9 permits per month and the majority of counties issue less than 40 permits per month. Additional data were obtained from the Bureau of Labor Statistics Quarterly Census of Earnings and Wages, including establishment counts and average weekly wages for the construction, hospitality and recreation industries at the county-level. We use construction industry characteristics to test for the possibility of spatial information and production spillovers that may bias the estimated effects of the SLR report on NC coastal housing development.

For our parcel-level analysis, data on individual building permits for new homes come from the Dare County Planning Office covering 2005 to 2017. These data contain the specific geographic location of each parcel and the estimated construction cost of each permitted activity. Combining this data with NOAA predictions of SLR, we assign each new permit location to a SLR risk based on predicted inundation (0.3 to 1.2 m). We use this fine-scale data to test the effect of the SLR report and subsequent repeal on housing development within risk zones where future regulations are most likely (Fig. 3).

3.1 Empirical Models

The fundamental difficulty in causal analysis is the failure to observe the counterfactual outcome of a treated unit had treatment not occurred. In the experimental ideal, understanding the effects of a climate adaptation policy signal on housing development would be accomplished by randomizing the signal across similar counties and comparing outcomes before and after. With the release of the CRC’s SLR report, information about the potential for future land-use regulations to mitigate SLR risk entered the public domain but only NC coastal counties are subject to regulatory changes from the issuing agency. Therefore, we define the 20 NC coastal counties subject to regulatory authority under the DCM as the treatment group and outcomes are observed before and after the release of the SLR report; however, it is not possible to observe the outcome in NC coastal counties had the treatment not taken place. To overcome this issue, we employ a quasi-experimental econometric framework and construct counterfactual control groups using data from non-coastal NC counties and both coastal and non-coastal counties in a neighboring state (SC).

Given that housing starts are count distributed, it is likely that estimating our models with ordinary least squares would be biased. The most common methods used in empirical analysis to model count data are the Poisson and negative binomial models [57]. Both are well suited for modeling count distributions however
the Poisson model requires the restrictive assumption of mean variance equality. The negative binomial model flexibly models variance thereby accommodating overdispersion, a common occurrence in count data where the data have variance greater than the mean. The negative binomial distribution is given by:

\[ f(y_i; \mu, \alpha) = \frac{\Gamma(\alpha^{-1} + y)}{\Gamma(\alpha^{-1})\Gamma(y + 1)} \left( \frac{\alpha^{-1}}{\alpha^{-1} + \mu} \right)^{\alpha^{-1}} \left( \frac{\mu}{\mu + \alpha^{-1}} \right)^{y_i}, \]  

where \( \Gamma(y+1) = (y+1)! \), \( \mu \) is a deterministic function of the regressors \( (x) \) and \( \alpha \) is the dispersion parameter. This yields the following properties:

\[
E[y|\mu, \alpha] = \mu, \\
V[y|\mu, \alpha] = \mu(1 + \alpha \mu). 
\]

Estimating models using a negative binomial distribution follows from the standard assumption of exponential mean parameterization:

\[ \mu_i = \exp(x'_i \beta), \]

where \( \beta \) represents the vector of estimated coefficients that include the treatment effect parameters in our empirical model specifications.

Our data provide an opportunity to exploit both the timing of the policy signals and the spatial variation in jurisdictions where such policies would be expected. We use a difference-in-difference-in-differences, or triple differences, estimator to control for changes in housing starts across states and coastal areas independent of the policy signal and changes in housing starts resulting from other state-specific changes in the economy. For example, it is possible that there are systemic differences between not only North Carolina and neighboring states but also the coastal and non-coastal policies in those states and our specification can control for such factors. Our preferred specification uses South Carolina as the neighboring state due to the shared border between the states, the similarities in coastal geography, and the fact that coastal counties in the Carolinas are very similar in terms of population density and income. We estimate the effect on housing starts of both the SLR report release and subsequent repeal as follows:

\[
y_{it} = \beta_0 + \beta_1 During_{it} + \beta_2 Repeal_{it} + \beta_3 (NC_i \times During_{it}) + \beta_4 (Coastal_i \times During_{it}) + \\
\quad + \beta_5 (NC_i \times Repeal_{it}) + \beta_6 (Coastal_i \times Repeal_{it}) + \beta_7 (NC_i \times Coastal_i \times During_{it}) + \\
\quad + \beta_8 (NC_i \times Coastal_i \times Repeal_{it}) + County_{it} + (County_{it} \times Trend_{t}) + Year_t + \epsilon_{it},
\]

where \( y_{it} \) is permits issued for new single family home starts in county \( i \) in month \( t \), where \( NC_i = 1 \) for NC counties, \( Coastal_i = 1 \) for all coastal counties and \( During_{it} = 1 \) for \( t \in \{March 2010, August 2012\} \) and \( Repeal_{it} = 1 \) after August 2012 and H.B. 819. \( County_{it} \) and \( Year_t \) are county and year fixed effects respectively, \( Trend \) is a linear time trend and \( \epsilon_{it} \) is the disturbance term. The parameters of interest are \( \beta_7 \) and \( \beta_8 \), which estimate the average treatment effect of the SLR report release and repeal, respectively.

To test the robustness of our triple differences specification, we also estimate a difference-in-differences (DiD) model comparing pre- and post-treatment for coastal counties in NC as compared to non-coastal NC
where $Treated_i = 1$ for NC coastal counties

For the parcel-level analysis in Dare County, NC, we estimate the following DiD model, where $y_{it}$ is the number of housing starts in zone $z$ during month $t$ and $Treated_z = 1$ for permits issued within a defined SLR risk zone,

$$y_{zt} = \beta_0 + \beta_1 Treated_z + \beta_2 (Treated_z \times During_t) + \beta_3 (Treated_z \times Repeal_t) + Year_t + \epsilon_{zt}.$$  \hfill (11)

We estimate Equation (11) for parcels at risk of inundation from SLR up to 1.2 m, consistent with the information contained in the SLR report. As a robustness check, we estimate separate models for three additional risk zones (0.3 m, 0.6 m, and 0.9 m) to approximate the average effect on housing development in higher SLR risk areas in Dare County.

### 3.2 Identification

In the linear framework, pre-treatment parallel trends is used to evaluate the comparability of treatment and control groups in a quasi-experimental analysis. Parallel trends tests are commonly performed with a visual inspection by plotting the dependent variable over time; however, in the non-linear context, parallel trends in the outcome variable for treatment and control groups in the pre-treatment period does not imply identification \[58, 59\]. Lechner (2010) demonstrates that parallel trends in the raw outcome variable results in bias in treatment effect estimates. Therefore, we follow the approach of Muralidharan and Prakash \[60\] and empirically test the parallel trends assumption by estimating triple-differences and DiD models using data for the pre-report period and interacting the treatment indicators, $NC_i \times Coastal_i$ and $Coastal_i$, with a continuous measure of time, $Trend_t \in [1, 38]$ representing the 38 months in our sample prior to the report release. This specification tests for parallel trends in an additive linear index of the outcome variable, the assumption necessary for identification in a non-linear framework \[59\]. Specifications for the empirical tests are:

$$y_{it} = \beta_0 + \beta_1 (NC_i \times Coastal_i \times Trend_t) + \beta_2 (NC_i \times Trend_t) + (County_i \times Trend_t) + Year_t + \epsilon_{it}.$$  \hfill (12)

$$y_{it} = \beta_0 + \beta_1 (Coastal_i \times Trend_t) + \beta_2 Trend_t + County_i + (County_i \times Trend_t) + Year_t + \epsilon_{it}.$$  \hfill (13)

where equation 12 and 13 correspond to the triple-differences and DiD specifications respectively. If the interaction coefficient $\beta_1$ for $(NC_i \times Coastal_i \times Trend_t)$ and $(Coastal_i \times Trend_t)$ is statistically different from
zero, we may reject the parallel trends assumption. Table 1 (model 1) contains results for the parallel trends test estimated using equation 12 in the triple-differences framework. We fail to reject the null hypothesis ($H_0 : \beta_1 = 0$), offering evidence that trends are similar in the pre-report period across the two groups. A similar test for the DiD models comparing coastal and non-coastal NC counties is displayed in Table 1 models 2 and 3 displays the results of comparing trends for homes permitted in the 1.2 m SLR risk zone to all other permitting in Dare County. The coefficient of interest in the county-level DiD model ($\text{Coastal} \times \text{Trend}$) is not statistically different from zero, suggesting similar trends in Coastal and non-coastal NC counties in the pre-report period. For the parcel-level analysis, the 1.2m $\text{SLR} \times \text{Trend}$ coefficient is not statistically significant at the five percent level and the magnitude is very small. Therefore we fail to reject the parallel trends assumption for both the triple differences and DiD county- and parcel-level models at the five percent level.

4. Results

We find empirical evidence that the expectation of forthcoming land-use regulations from a science-based policy signal intended to minimize future SLR damages resulted in an immediate, quantifiable increase in development of at-risk land - a variant of the green paradox. Our estimates suggest that average monthly housing starts increased by approximately 32% between March 2010 (report release) and August 2012 (report repeal) ([IRR] Table 2, models 1 & 2). The estimates are nearly identical for the DiD model using just NC counties. The implication of our results is that the SLR report was responsible for an increase of approximately 7.8 additional homes being constructed monthly in each county, totaling over 3,150 new homes during the 29 month period between release and repeal of the report.

This represents a temporal leakage of future, potentially regulated, development that is pulled into the current unregulated period due to the policy signal. It is possible that this increase is the result of a harvesting effect, whereby developers construct new homes now that would have been developed anyway at some future date, in the absence of expected policy changes. As argued earlier, this interpretation misses a key point - that even if harvesting is occurring, it is increasing the stock of unregulated construction and increasing overall vulnerability to SLR. Furthermore, if harvesting was the mechanism at play, we would expect to find a drop in housing starts relative to the baseline trend after policymakers placed a moratorium on using the SLR report as the basis for any new regulations in August 2012. We do not find evidence of this effect during our sample, given that new housing starts after H.B. 819 is passed are still 6% to 9% above baseline but not statistically significant. It is possible that our sample time-frame is too short to measure a harvesting effect. Our evidence suggests that developers responded to the regulatory signal by adopting a “build while you can” strategy and then returned to business as usual after passage of H.B. 819. The

$^6$In the context of this study incident rate ratios (IRR) for the negative binomial are interpreted as $\text{Treated Outcome} = \text{IRR} \times \text{Counterfactual Outcome}$.
return to baseline trend after report repeal offers further evidence that developers internalize H.B. 819 as a decrease in the probability of higher regulatory intensity ($\gamma_{\ell+1}$, eq. [4]). Increased construction activity in high risk zones, whether the result of harvesting or policy-induced development decisions, is counter to the stated goals of the regulator and results in this green paradox outcome in a coastal climate adaptation setting.

4.1 Parcel-level effects across in Dare County

To examine responses to the SLR report release and repeal at a more granular spatial scale, we use parcel-level data on housing starts in Dare County, NC. This county contains over 100 miles of shoreline along narrow barrier islands, where the majority of parcels were subdivided and developed prior to the SLR report release. This lessens concerns about permitting lags for large subdivisions confounding the timing of our research design. We assigned each parcel to a risk zone using NOAA estimates of projected SLR inundation. For example, $Treated = 1$ if a permit is issued for a parcel that would be inundated under a 1.2 m SLR scenario. Estimating equation [11], we find an economically and statistically significant increase in housing starts in the 1.2 m risk zone of 42%. Similar to our county-level analysis, we find evidence that development activity returns to business as usual after the SLR report is repealed as shown by the $(Treated \times Repeal)$ coefficient in Table [2] (model 3).

4.2 Projected increase in vulnerable assets

The intention of the 2010 CRC report was to mitigate future damages to property and infrastructure by incorporating SLR estimates into land-use planning and building regulations. Yet the lack of concurrent political action to implement new policies created economic incentives that led to a green paradox. Here we use our econometric model estimates to quantify a back-of-the-envelope approximation of the magnitude of the paradoxical effect of the SLR report. This is not the full social cost of the policy signal, but represents a short term increase in at-risk assets built without regulations aimed at making the structures resilient to future SLR. Between March 2010 and August 2012, the average NC coastal county issued 32 permits for new single-family homes each month, which was 32% higher than business as usual. In the absence of the policy signal, we would have expected 24.2 permits to be issued, implying the impact of the SLR report was an additional 7.8 housing starts in each county per month. Given an average construction cost associated with each permit of approximately $126,000, our result implies that the average NC coastal county saw new home construction activity increase by approximately $983,000 per month as a direct result of the policy signal. Summing this effect across the 14 coastal counties in the sample, for 29 months between report release and repeal, equates to an increase in single family home construction attributable to the SLR report of $399 million. This estimate represents a lower bound of the value of the increase in new housing stock as it is a measure of construction costs only. We use parcel level data from Dare County to measure the
relationship between sales price and estimated cost of construction, finding that the estimated construction cost represents approximately 56.5% of a home’s sale price. This is consistent with survey data over the last 20 years from the National Association of Home Builders (NAHB), suggesting that construction costs represent an average of 55.8% of sales prices. This implies the economic value of new homes constructed in coastal NC as a result of the report may be closer to $715 million, or $24.7 million per month.

4.3 Robustness checks

We confirm the robustness of our results from our county-level triple differences model by conducting placebo tests varying the timing of a hypothetical report to test for unobserved shocks impacting housing starts, modeling the potential for information and production spillovers across jurisdictions, estimating models with alternative control groups, and varying the SLR risk zone definition in our parcel-level models.

**Placebo Test**

We first test for unobserved confounders by dropping data after initiation of the treatment (March 2010) and imposing a placebo treatment in March 2009. We estimate the following triple differences model and equivalent difference-in-differences model:

\[
y_{it} = \beta_0 + \beta_1 \text{PostMarch2009}_t + \beta_2 (\text{NC}_i \times \text{PostMarch2009}_t) + \beta_3 (\text{Coastal}_i \times \text{PostMarch2009}_t) + \beta_4 (\text{NC}_i \times \text{Coastal}_i \times \text{PostMarch2009}_t) + \text{County}_i + (\text{County}_i \times \text{Trend}_i) + \text{Year}_t + \epsilon_{it},
\]

where PostMarch2009\(_t\) = 1 if \(t \in [\text{March 2009, March 2010}]\). Results shown in Table 3 suggest that no unobserved confounders affected housing starts in the period leading up to the SLR report release (i.e., coefficients for NC\(_i\) × Coastal\(_i\) × PostMarch2009\(_t\) and Treated × PostMarch09 are not significantly different from zero). There is no evidence of higher permitting along the North Carolina coast from 2009 to 2010, suggesting no differential impacts in coastal counties in the pre-report period. These results are robust to imposing placebo treatments both six (6) and eighteen (18) months before the report release and repeating the analysis.

**Spatial Spillovers**

There may be concern that two states which share a border, such as North and South Carolina, may not be two separate housing markets. Specifically, it is possible that activity in one market may affect another, which could confound identification of treatment effects. There are two mechanisms through which spatial spillovers may bias our results. The first is the presence of information spillovers. If the NC SLR report updated expectations of SLR risk in South Carolina, we would expect to see a measurable effect in SC coastal communities. We do not believe there is a risk of information spillover for two reasons. First,

---

7Using data from Dare County we observe the difference in estimated construction cost and observed sales for homes that were sold within 2 years of construction.
the NC SLR report contained no new science, i.e., it was drawn from existing IPCC SLR estimates and synthesized specifically for the North Carolina coast. Second, the report was intended to be used for NC regulatory purposes, such as land-use planning and infrastructure design and this should have no impact on the regulatory environment in SC.

The second mechanism involves relocating productive assets from SC to NC as a part of a “build while you can” strategy. If residential construction firms operate in both states, there is the possibility for substitution. If firms shift construction from SC to NC, this would lead to an increase in housing starts in NC and a decrease in SC. We test for potential production spillovers by estimating DiD models for SC housing starts (Table 4). Model (1) drops all data after report repeal and estimates a DiD specification with SC coastal counties as the treated group and SC non-coastal counties as the counterfactual control group and model (2) includes both the report release and repeal treatments. Results suggest no change in housing starts along the SC coast coinciding with the report release as compared to non-coastal SC counties. If production spillovers were a valid concern, we would expect housing starts to decrease concurrent with the report release.

To further explore production spillovers, we use data from the Quarterly Census of Earnings and Wages compiled by the U.S. Bureau of Labor Statistics. We estimate DiD models using residential construction establishment counts to evaluate structural changes in NC and SC coastal economies. Evaluating the Coastal \times During parameter in table 5 we see that establishment counts decreased in the coastal regions of North and South Carolina between March 2010 and August 2012, however there is no evidence of production moving from SC to NC.

Alternative County Controls

We estimate additional triple differences models (equation 9) using observations in VA as part of the control group due to shared border of the two states to test sensitivity of our main results to this choice. It is important to note, however, that coastal counties in VA include urban and suburban areas near Washington D.C. and the Hampton Roads Metropolitan Statistical Area. Furthermore, VA coastal counties have nearly 9 times the population, 4 times the density, and 30 percent more household income than NC counties. Table 6 contains triple-differences estimates using Virginia only (model 1), and South Carolina and Virginia (model 2), as the counterfactual, we also include a DiD estimates (model 3) restricting the sample to coastal counties in North Carolina and South Carolina, where Treated = 1 for NC coastal counties. While effects in the post-repeal period differ when including Virginia in the triple-differences model, the sign and magnitude of the estimated treatment effect while the regulatory signal was relevant is consistent with our preferred specification (Table 5 model 1). Furthermore, model 3 estimates are consistent with our preferred specification for both report release and repeal. Importantly, we see no statistically significant evidence that housing starts decreased below baseline levels in the post-repeal period, further reinforcing our temporal leakage argument and green paradox conclusion.
Alternative Dare County Risk Zones

In the parcel-level models, we evaluate additional SLR risk zones denoted by 0.3, 0.6 and 0.9 m inundation projections. We estimate equation 11 separately for each risk zone, i.e., in the 0.3 m Zone model in table 8

\[ Treated = \begin{cases} 
1 & \text{if parcel inundated under 0.3m SLR} \\
0 & \text{otherwise} 
\end{cases} \]

First, it is important to note that defining treatment and control groups across 0.3 to 0.9 m SLR risk zones fails our empirical parallel trends test (Table 7); however, we include these estimates to evaluate the sign and magnitude of coefficients for parcels with higher SLR risk. Model estimates shown in Table 8 point to a potentially stronger response to the policy signal, with development increasing up to 77 percent in areas vulnerable to smaller amounts of SLR. We also see evidence of housing starts remaining elevated after the report is repealed, especially in the 0.3 and 0.6 m areas, which is consistent with the fact the H.B. 819 suggested an extended linear trend (15 inches of SLR) should be used as the basis for new regulations. However, the failure of 0.3 - 0.9 m risk zones models to empirically demonstrate parallel trends in the pre-report period suggests we should interpret these results with care as they are estimated with models that violate a necessary assumption. Note that as we increase the size of the SLR risk zone to include less risky parcels, i.e., those between the 0.9 and 1.2 m risk zones, the size of the treated sample increases and evidence suggests we are constructing a more plausible control group. The important message here is that the sign of all coefficient in Table 8 are the same as our preferred triple-differences specification shown in Table 2 further supporting the robustness of our main finding. Given our current data constraints, untangling the effects of expected policy changes across SLR risk zones remains as an area for future research.

5. Conclusion

Credible and salient scientific information about climate risks is needed to inform better adaptation policies. Our work provides a note of caution to both scientists and policy-makers that communication of science-based estimates of risks by regulatory agencies can create perverse incentives that may undermine policy goals. We present evidence suggestive of a green paradox in climate adaptation planning and quantify the potential magnitude of the economic impacts of the paradox. We utilize an exogenous information shock with differential spatial implications across jurisdictional boundaries to identify a plausibly causal link between housing starts and a policy signal suggesting future development regulations to mitigate risk from SLR. We find that this science-based policy signal aimed at restricting risky development and making communities more resilient to climate change caused a significant increase in housing starts in risky areas of the North Carolina coast. The driver of this result is likely due to expectations of a reduction in option value or of costlier development in the future created by the policy signal that was not accompanied by concurrent political action. Our hypothesis is supported by both a county-level analysis using jurisdictions outside of
coastal NC as controls and a parcel-level analysis comparing housing starts across SLR risk zones within the same county, along with a suite of robustness checks.

Our results suggesting an economically and statistically significant temporal leakage of development decisions from future regulated time periods resulted in an immediate increase in the value of risky housing stock along the NC coast. This has implications beyond increasing potential damages from SLR. A report released by CoreLogic in the aftermath of Hurricane Florence (2018) suggests property damages in NC may reach as high as $22 billion from flooding related to the storm. Although we cannot directly attribute increases we found in housing stock to damages from the storm, intuition suggests that an increase in less resilient development in these areas opens up the potential for increasing property losses from hurricane events. Furthermore, shoreline management decisions and program budgets are inextricably tied to the value of coastal housing stocks. For instance, as the value of these assets increase, the incentive to engage in beach nourishment has also been shown to increase [62]. While such federal interventions provide protective benefits to coastal homeowners, they are expensive undertakings and the costs may exceed social benefits [28]. Additionally, this paradoxical outcome increases the amount of property covered by the National Flood Insurance Program and the liabilities of the Federal Emergency Management Agency in providing disaster aid. Our estimation of the magnitude of the green paradox suggests a substantive increase in liabilities in North Carolina for the federal government, potentially compounding the impacts associated with the policy signal and subsequent inaction. Lastly, more construction in vulnerable coastal areas likely reduces valuable ecosystem service flows (e.g., habitat, storm protection) and limits future adaptation options, as increases in development may make managed retreat less likely [24].

Integration of science and policy initiatives is a grand challenge for societies facing a myriad of issues related to climate change. Behavioral responses to expectations of new regulations, such as those shown here, should be of broad interest to policy-makers when developing new regulatory regimes to adapt to climate futures. Good science is needed to inform policy but the political system must be willing to act on that information in a credible and timely fashion. Without concurrent action on these two fronts, unintended behavioral responses by individuals to economic incentives has the potential to undermine policies aimed at protecting collective public interests.

Data Availability

All data and code necessary for replication of the results in this article are available for download at https://figshare.com/s/4d6699b20eb64aece769

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URL http://www.nahbclassic.org/generic.aspx?genericContentID=260013

Figure 1: Relative Sea-Level Rise Projections for North Carolina through 2100. This figure is reproduced from the North Carolina CRC SLR report ([50]) released in March 2010.
Figure 2: Timeline of report events. The CRC SLR report was released in March 2010 and repealed in August 2012 by H.B. 819. In the period between these two events there was a high level of uncertainty about if, when, and how new land-use regulations designed to reduce SLR risks would be implemented.
Figure 3: Potential impacts of sea-level rise on Dare County, NC. Panel A illustrates the 20 NC coastal counties under the purview of the NC CRC where new regulations were expected after the release of the SLR report in March 2010. Panel B outlines Dare County, the location for our parcel-level analysis. Panel C depicts a cross-section of Dare County (near the town of Nags Head) and includes shading indicating inundation of land from 1.2 m (approximately 4 ft) of SLR.
Table 1: Empirical test of county level parallel trends.

<table>
<thead>
<tr>
<th>Triple differences test</th>
<th>Difference-in-differences tests</th>
</tr>
</thead>
<tbody>
<tr>
<td>South Carolina</td>
<td>North Carolina</td>
</tr>
<tr>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>( Coastal \times NC \times Trend )</td>
<td>( Coastal \times Trend )</td>
</tr>
<tr>
<td>( )</td>
<td>( 0.0095 )</td>
</tr>
<tr>
<td>( )</td>
<td>( (0.0094) )</td>
</tr>
<tr>
<td>( NC \times Trend )</td>
<td>( -0.0043 )</td>
</tr>
<tr>
<td>( )</td>
<td>( (0.0037) )</td>
</tr>
<tr>
<td>( Coastal )</td>
<td>( -4.117^{***} )</td>
</tr>
<tr>
<td>( )</td>
<td>( (0.213) )</td>
</tr>
<tr>
<td>( Trend )</td>
<td>( -0.036^{***} )</td>
</tr>
<tr>
<td>( )</td>
<td>( (0.0036) )</td>
</tr>
<tr>
<td>( NC )</td>
<td>( -0.794^{***} )</td>
</tr>
<tr>
<td>( )</td>
<td>( (0.117) )</td>
</tr>
</tbody>
</table>

\[ N \] \hspace{1cm} 3940 \hspace{1cm} 2660 \hspace{1cm} 122

Notes: The dependent variable is count of monthly housing starts. Data after the SLR report release is dropped and the triple differences and difference-in-differences parallel trend assumption is tested by interacting treatment indicators with a linear time trend. Standard errors reported in () are clustered at the county level for models 1 & 2 and robust for model 3. Significance denoted by \( ^* p < 0.10, ~ ^{**} p < 0.05, ~ ^{***} p < 0.01. \)
Table 2: Treatment effects estimates

<table>
<thead>
<tr>
<th></th>
<th>South Carolina</th>
<th>North Carolina</th>
<th>Dare County</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>Est</td>
<td>[IRR](^a)</td>
<td>Est</td>
<td>Est</td>
</tr>
<tr>
<td>( NC \times Coastal \times During )</td>
<td>0.276** [1.317]</td>
<td>( Treated \times During )</td>
<td>0.272** [1.312]</td>
</tr>
<tr>
<td></td>
<td>(0.140)</td>
<td>(0.117)</td>
<td>(0.149)</td>
</tr>
<tr>
<td>( NC \times Coastal \times Repeal )</td>
<td>0.0626 [1.064]</td>
<td>( Treated \times Repeal )</td>
<td>0.0963 [1.098]</td>
</tr>
<tr>
<td></td>
<td>(0.236)</td>
<td>(0.111)</td>
<td>(0.101)</td>
</tr>
<tr>
<td>Year Fixed Effect</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>County Fixed Effect</td>
<td>Y</td>
<td>Y</td>
<td>N</td>
</tr>
<tr>
<td>(County×Time Trend)</td>
<td>Y</td>
<td>Y</td>
<td>N</td>
</tr>
<tr>
<td>ln((\alpha))</td>
<td>-2.319***</td>
<td>-2.293***</td>
<td>-4.38***</td>
</tr>
<tr>
<td></td>
<td>(0.0797)</td>
<td>(0.0873)</td>
<td>(0.786)</td>
</tr>
<tr>
<td>(N)</td>
<td>10860</td>
<td>2758</td>
<td>312</td>
</tr>
</tbody>
</table>

Notes: The dependent variable is count of monthly housing starts for all models. In model 3, \( Treated = 1 \) if a given parcel in Dare County is located would be inundated under 1.2 m of SLR. Standard errors reported in () are clustered at the county level for models 1 & 2 and robust for model 3. Significance denoted by * \( p < 0.10 \), ** \( p < 0.05 \), *** \( p < 0.01 \)

\(^a\) Corresponding incident rate ratios (IRR) are reported in [ ]. IRR treatment effects are given by \( Treated \text{ Outcome} = IRR \times Counterfactual \text{ Outcome} \)
Table 3: Housing starts placebo tests.

<table>
<thead>
<tr>
<th></th>
<th>South Carolina</th>
<th>North Carolina</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>NC × Coastal × PostMarch09</td>
<td>-0.0515</td>
<td>0.0249</td>
</tr>
<tr>
<td></td>
<td>(0.15)</td>
<td>(0.085)</td>
</tr>
<tr>
<td>Year Fixed Effect</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>County Fixed Effect</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>(County × Time Trend)</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>ln(α)</td>
<td>2.651***</td>
<td>-2.588***</td>
</tr>
<tr>
<td></td>
<td>(0.080)</td>
<td>(0.095)</td>
</tr>
<tr>
<td>N</td>
<td>4043</td>
<td>2730</td>
</tr>
</tbody>
</table>

Notes: The dependent variable is count of monthly housing starts. Data after the SLR report is released in March 2010 are dropped, PostMarch09 = 1 for observations after March 2009. Standard errors clustered at the county level reported in (), significance denoted by * p < 0.10, ** p < 0.05, *** p < 0.01.
Table 4: Effect of the SLR report on SC housing starts.

| Difference-in-differences | South Carolina |
|---------------------------|--|---|---|
|                           | Release Only | Release & Repeal |
| Coastal × During          | -0.0374      | -0.0022     |
|                           | (0.0675)     | (0.0617)    |
| Coastal × PostRepeal      | 0.0337       |             |
|                           | (0.1001)     |             |
| Year Fixed Effect         | Y            | Y           |
| County Fixed Effect       | Y            | Y           |
| (County × Time Trend)     | Y            | Y           |
| Observations              | 2270         | 3602        |

Notes: Dependent variable is count of monthly housing starts in South Carolina, for model (1) data are dropped after report repeal (August 2012) and equation 10 is estimated excluding PostRepeal. Standard errors clustered at the county level reported in (), significance levels denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Table 5: Effects of the SLR report on the residential construction industry.

<table>
<thead>
<tr>
<th></th>
<th>South Carolina</th>
<th></th>
<th>North Carolina</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Release Only</td>
<td>Release &amp; Repeal</td>
<td>Release Only</td>
<td>Release &amp; Repeal</td>
</tr>
<tr>
<td>------------------------------</td>
<td>----------------</td>
<td>-------------------------------------------</td>
<td>----------------</td>
<td>-------------------------------------------</td>
</tr>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
</tr>
<tr>
<td><em>DuringReport</em> × Coastal</td>
<td>-0.0357**</td>
<td>-0.036***</td>
<td>-0.0612***</td>
<td>-0.0536***</td>
</tr>
<tr>
<td></td>
<td>(0.0153)</td>
<td>(0.014)</td>
<td>(0.0096)</td>
<td>(0.0104)</td>
</tr>
<tr>
<td><em>PostRepeal</em> × Coastal</td>
<td>0.0928***</td>
<td></td>
<td>-0.0676***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.0151)</td>
<td></td>
<td>(0.0093)</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>1457</td>
<td>2436</td>
<td>3131</td>
<td>5252</td>
</tr>
</tbody>
</table>

Notes: The dependent variable is monthly count of residential construction establishments by county. Equation [10] is estimated, dropping post-repeal data for models 1 & 3. We independently test for changes in SC coastal establishment counts (models 1 & 2) and NC coastal establishments (models 3 & 4). Standard errors clustered at the county level reported in ( ), significance denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. 
Table 6: Treatment effects estimates using alternative controls.

<table>
<thead>
<tr>
<th></th>
<th>Triple Differences</th>
<th>Difference-in-Differences</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>VA (1)</td>
<td>SC &amp; VA (2)</td>
</tr>
<tr>
<td>$NC \times Coastal \times During$</td>
<td>0.345*** (0.148)</td>
<td>0.304** (0.137)</td>
</tr>
<tr>
<td>$NC \times Coastal \times Repeal$</td>
<td>0.389** (0.169)</td>
<td>0.258* (0.139)</td>
</tr>
<tr>
<td>Year Fixed Effect</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>County Fixed Effect</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>(County x Time Trend)</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>$\ln(\alpha)$</td>
<td>-2.08*** (0.075)</td>
<td>-2.128*** (0.071)</td>
</tr>
<tr>
<td>$\hat{N}$</td>
<td>15766</td>
<td>19368</td>
</tr>
</tbody>
</table>

Notes: The dependent variable is count of housing starts for all models. Model 3 is estimated by dropping all non-coastal observations and estimating Equation 10 using NC coastal counties as the treated group and SC coastal counties as counterfactual controls. Standard errors clustered at the county level reported in (), significance denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Table 7: Empirical test of Dare County parallel trends by risk zone.

<table>
<thead>
<tr>
<th></th>
<th>0.3m Zone (1)</th>
<th>0.6m Zone (2)</th>
<th>0.9m Zone (3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>$RiskZone \times Trend$</td>
<td>0.0224***</td>
<td>0.0184***</td>
<td>0.0136***</td>
</tr>
<tr>
<td></td>
<td>(0.00378)</td>
<td>(0.00334)</td>
<td>(0.00363)</td>
</tr>
<tr>
<td>$RiskZone$</td>
<td>-1.454***</td>
<td>-0.874***</td>
<td>-0.00372</td>
</tr>
<tr>
<td></td>
<td>(0.120)</td>
<td>(0.0978)</td>
<td>(0.0967)</td>
</tr>
<tr>
<td>$Trend$</td>
<td>-0.0318***</td>
<td>-0.0323***</td>
<td>-0.0326***</td>
</tr>
<tr>
<td></td>
<td>(0.00336)</td>
<td>(0.00379)</td>
<td>(0.00709)</td>
</tr>
<tr>
<td>Observations</td>
<td>124</td>
<td>124</td>
<td>124</td>
</tr>
</tbody>
</table>

Robust standard errors reported in ()

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The dependent variable is count of monthly housing starts. Data after the SLR report is released is dropped and $RiskZone$ is interacted with a linear time trend to test the parallel trend assumption. Robust standard errors reported in (), significance denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. 
Table 8: Dare County parcel level treatment effects by SLR risk.

<table>
<thead>
<tr>
<th></th>
<th>0.3m Zone</th>
<th>0.6m Zone</th>
<th>0.9m Zone</th>
<th>1.2m Zone</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
</tr>
<tr>
<td>RiskZone × During</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Est</td>
<td>0.571***</td>
<td>0.534***</td>
<td>0.458***</td>
<td>0.354**</td>
</tr>
<tr>
<td>[IRR]</td>
<td>[1.770]</td>
<td>[1.706]</td>
<td>[1.582]</td>
<td>[1.424]</td>
</tr>
<tr>
<td>(Est)</td>
<td>(0.151)</td>
<td>(0.268)</td>
<td>(0.137)</td>
<td>(0.229)</td>
</tr>
<tr>
<td>RiskZone × Repeal</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Est</td>
<td>0.583***</td>
<td>0.431***</td>
<td>0.172*</td>
<td>0.0393</td>
</tr>
<tr>
<td>[IRR]</td>
<td>[1.792]</td>
<td>[1.538]</td>
<td>[1.187]</td>
<td>[1.040]</td>
</tr>
<tr>
<td>(Est)</td>
<td>(0.104)</td>
<td>(0.186)</td>
<td>(0.0979)</td>
<td>(0.116)</td>
</tr>
<tr>
<td>Year Fixed Effect</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>ln((alpha))</td>
<td>-3.665***</td>
<td>-3.520***</td>
<td>-3.624***</td>
<td>-4.381***</td>
</tr>
<tr>
<td>(Est)</td>
<td>(0.457)</td>
<td>(0.392)</td>
<td>(0.426)</td>
<td>(0.786)</td>
</tr>
<tr>
<td>N</td>
<td>312</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Notes: The dependent variable is count of monthly housing starts. RiskZone = 1 for parcels located within a given SLR risk zone. Robust standard errors reported in (). * \(p < 0.10\), ** \(p < 0.05\), *** \(p < 0.01\).

\(a\) Corresponding incident rate ratios (IRR) are reported in [ ]. IRR treatment effects are given by \(Treated\ Outcome = IRR \times Counterfactual\ Outcome\).